II. Account of Observations at the Cambridge Observatory for detecting the Planet exterior to *Uranus*. By Professor Challis.*

After some preliminary remarks, Professor Challis says,—"In September, 1845, Mr. Adams, Fellow of St. John's College, Cambridge, placed in my hands a paper containing the results of computations he had made to account for irregularities in the motion of *Uranus*, on the hypothesis of disturbances caused by a more distant planet. These results embraced the mass, mean distance, mean longitude at a given epoch, longitude of perihelion, and excentricity, of the orbit of the disturbing body, with its probable geocentric longitude at the end of September." The time was unfavourable for looking after the planet, on account of its angular distance from opposition; but the main reason which deterred the Professor from the search was, that it was "so novel a thing to undertake observations in reliance upon merely theoretical deductions, and that while much labour was certain, success appeared very doubtful." †

The publication of M. Le Verrier's first memoir, and the close agreement of his deductions with those of Mr. Adams, together with the recommendation of the Astronomer Royal, induced Professor Challis to undertake the search of the predicted planet.‡ The observations were commenced July 29, 1846, three weeks before the expected opposition of the planet.

In suggesting a plan for finding the planet, Mr. Airy advised the close scrutiny of a zodiacal zone, having its centre in the ecliptic at 325° of longitude, (the point marked out by Mr. Adams and M. Le Verrier) extending each way 15° of longitude and 5° of latitude. A power not lower than 120 was advised, with which a zone of 15′ would be well seen; also that the transits should be taken at one wire; and the north polar distances estimated in parts of intervals of 3′, marked by horizontal wires. It was calculated that 80 sweeps, each averaging one hour, repeated three times, would occupy about 300 hours of observing weather. In this mode of observing, the telescope is supposed to be "fixed" during the whole, or at least during the consecutive portions of each sweep.

Professor Challis adopted this plan with the following modifications,—he used a power of 166, having found experimentally this to be the power most comfortable for vision. This gave a field of 9′. The transits were taken at the edge of the micrometer, ascertained to be quite straight, and the declination from the teeth of the comb, noting by estimation the fourth or even tenth of a tooth, each tooth or revolution of the micrometer screw being equal to 17″. All stars were observed which were well visible in a mo-

* This paper was presented on the evening of Nov. 13, 1846.

† M. Le Verrier published his first determination of the probable longitude of the exterior planet in June 1846, yet it does not appear that any systematic attempt was made elsewhere to detect it by observation for nearly four months, nor until he had published his second determination.

‡ Mr. Airy's suggestion was made in a note dated July 9, 1846.

derately illuminated field, *i. e.* all stars to the 11th magnitude inclusive. But as such stars are very numerous, and frequently would have interfered with each other, another mode of observing was sometimes followed, which can only be successfully practised with an instrument of a similar construction with that employed.

The Northumberland equatoreal has, at its lower end, a complete hour circle, divided to 24 hours and moved uniformly by clockwork. This circle is wholly detached from the frame of the equatoreal, turning freely on the lower pivot, but the two can be attached at pleasure, when their relative positions can be altered at the will of the observer by a tangent screw, and measured by vernier readings or microscopes. If the hour circle and the clock be properly adjusted, the apparent right ascension of any object is read off at once upon the circle; and the error, either of the hour circle or clock, can be ascertained and eliminated by the observation of known stars. By the handle which governs the tangent screw, the observer is enabled to bring each star in order of right ascension to the meridian wire, and to give out the relative declination, magnitude, &c. to one assistant, who writes this down with the corresponding time by chronometer, while a second assistant reads off the apparent right ascension on the hour circle. The *known* stars thus observed furnish the correction of the declination wire, of the hour circle, and of the rate. Professor Challis distinguishes this from the ordinary method by the words "telescope moving."

On making trial of both methods, it was found that *more* stars were noted in the same *space*, but *fewer* stars in the same *time* when the observations were made "telescope moving." Hence, the Professor determined to go over the required space as carefully as possible, first in the ordinary method, and then a second time with the "telescope moving;" if the planet were not then discovered by its motion, it would, most probably, be found among the stars of the second operation which were not found in the first.

"The only reason I can give," says Professor Challis, "why I did not use hour XXI. of the Berlin star-maps is, that I was ignorant of its existence, as that hour was not to be found in the University Library, and consequently, as I believed, unpublished. If I had had this map a *first* sweep would have been unnecessary, I should have compared my field of view with the map at once."

The first observations were made on July 29, chiefly as a trial, but still carefully, and with the telescope turned to the most probable place of the expected planet. This place was taken from a paper by Mr. Adams, in which he had calculated the right ascension and declination of the planet for every twentieth day between July 20 and Oct. 8, and for every fifth degree of heliocentric longitude, from 315° to 335° inclusive. The places are deduced from theory, only assuming the planet to move in the ecliptic. The following is a specimen:—

For 325° of Heliocentric Longitude.

Date.	R. A. Planet.	Decl. Planet.
	h m	° ′
July 20	21 51·3	−13 0
Aug. 9	21 49·5	−13 9
29	21 47·9	−13 18
Sep. 18	21 46·5	−13 26
Oct. 8	21 45·1	−13 33

As the observations of July 29, with the telescope fixed, did not include all the stars which it seemed desirable to observe, the following observations on July 30 were made "telescope moving." For the next set, August 4, the telescope was moved in declination by the milled head of the differential sector, through 70′ and all the *brighter* stars were *carefully* observed in declination as well as in right ascension, to obtain places for correcting the partial zones of 9′. These declinations are probably correct to about 3″.

Moonlight and bad weather stopped proceedings till August 12. On this day the observations were made "telescope fixed," and the zone of July 30 repeated, taking in a larger extent in right ascension. Soon after, and probably next day (but there is no note of the exact time), the two series of July 30 and August 12 were partly compared, and so far as the comparison extended, every star taken on August 12 *was included* in the series of July 30, "telescope moving." Being thus practically convinced of the adequacy of the method, the Professor observed earlier in right ascension, so as to secure a scrutiny of the largest possible space this year. The observations were thus carried on for two months at every available opportunity, chiefly with the telescope fixed. On September 29, Professor Challis first read M. Le Verrier's communication of August 31, *in which he expressly recommends the endeavour to detect the planet by the appearance of a disk.* Mr. Adams had found the mass to be about three times that of *Uranus*, and had thence inferred that the brightness would not be below that of a star of the ninth magnitude. He had mentioned this to Professor Challis, who preferred, nevertheless, proceeding on his original plan, as less liable to ultimate disappointment.

The general agreement of M. Le Verrier's results respecting the mass and orbit of the unseen planet with those of Mr. Adams, as it gave greater confidence, induced Professor Challis again to modify his mode of observing. The evening of Sept. 29 was favourable, and a considerable breadth in declination was swept over within the limits of longitude pointed out by M. Le Verrier, paying particular attention to the physical appearance of the brighter stars, in accordance with his suggestion. Out of the 300 stars thus observed, only one attracted particular attention, and to this the assistant was directed to add the note, "It seems to have a disk." The observations could not be repeated the next night, and on October 1st information arrived that Dr. Galle had found the planet on September 23. Up to this time Professor Challis had noted 3000 positions of stars.

It was then found that the star selected for its appearance on

September 29 was the planet, thus verifying M. Le Verrier's remarkable prediction, "that the planet would be discoverable in a good telescope by its physical appearance."

On looking back at his earlier observations, Professor Challis had the vexation of finding that if he had compared *all* the observations of July 30 and August 12, the planet would then have been detected. The stars up to No. 39 of the series of August 12, "telescope fixed," were all found in the series taken July 30, "telescope moving." If this comparison had gone on a little further, it would have been found that No. 49 in the former series, *a star of the eighth magnitude, was wanting in the latter series.* From the principle of the search this must have been a planet. (It had, in fact, wandered into the zone between July 30 and August 12.) The comparison was really undertaken for another purpose, and probably was discontinued at that time after No. 39, as a cloud then interrupted the observations, after which a line of separation was drawn in the memorandum-book, and the clock was set going.

On a further examination, it was found that the planet was observed on August 4, when, as has been mentioned above, brighter stars were taken to serve hereafter as points of reference.

Professor Challis remarks that his oversight was partly caused by the pressure of comet reductions, but principally from an impression that a long search was required to ensure success. He was also anxious to secure the greatest number of observations, and so postponed the comparison till he had greater leisure. He admits, moreover, that he had too little confidence in the indications of theory, though perhaps as much as others might have felt in similar circumstances, and with similar engagements.

The observations, when carefully reduced, give the following places of the planet with considerable certainty:—

	Greenwich M. T.	Planet's R.A.	Planet's N.P.D.
	h m s	h m s	° ′ ″
Aug. 4	13 36 25	21 58 14·70	102 57 32·2
12	13 3 26	21 57 26·13	103 2 0·2

The errors of right ascension are probably not greater than those which belong to single transits. In north polar distance they are scarcely greater than 3″ on August 4, and 4″ on August 12.

Employing the foregoing observations, and others which extend to October 13, Mr. Adams finds the following elements of the planet's orbit:—

Heliocentric Longitude..................	326° 39′	August 4, 1846.
Longitude of the Descending Node...	309 43	
Inclination of the Orbit.................	1 45	
Distance of the Planet from the Sun	30·05	

The other elements do not admit, as yet, of any accurate determination. In remarking on the advantages of the mode of observing "telescope moving," Professor Challis says, "in the course of these observations, he could not help noticing that the

stars are more generally disposed in groups than scattered equally over the celestial spaces." To secure a complete survey of any portion of the heavens, this or some similar method seems almost necessary.

III. An Explanation of the observed Irregularities in the Motion of *Uranus*, on the Hypothesis of Disturbance caused by a more distant Planet; with a Determination of the Mass, Orbit, and Position of the disturbing Body. By J. C. Adams, Esq., M.A. F.R.A.S. Fellow of St. John's College, Cambridge.*

The author introduces the subject by remarking, that when Bouvard constructed his *Tables of Uranus* (those now commonly in use), he found it impossible to reconcile the ancient observations, made before the discovery of *Uranus* as a planet, with the modern observations, and that, therefore, in the formation of his tables, he relied solely upon the latter; but that, in a very few years, the still more modern observations exhibited a departure from the tables nearly as great as the ancient ones, and, therefore, there seemed now to be no sufficient reason for rejecting the ancient observations. The author then states, that his attention was first directed to this subject by reading the Report on the recent progress of astronomy made to the British Association, at their meeting in Oxford; and that in July, 1841, he formed a design of investigating the yet unaccounted-for motions of *Uranus*, in order to discover whether they could be explained by an exterior disturbing planet. In 1843 he made a first attempt, supposing the orbit of the disturbing planet to be a circle, and its mean distance twice that of *Uranus*. This investigation was founded exclusively on the modern observations, using, as far as 1821, the errors given in the equations of condition in Bouvard's tables, and for subsequent years the errors given in the *Astronomische Nachrichten*, and the Cambridge and Greenwich Observations. The result shewed a good general agreement of the observed disturbance with the disturbance which would be produced by the action of such a planet. In February, 1844, the author received from the Astronomer Royal the results of the general reduction of the *Greenwich Planetary Observations*.

In the meantime the Göttingen Academy had proposed for the subject of a prize the theory of *Uranus*, and though the author had no hope of being able to complete an essay in time to compete for the prize, he was stimulated by the publication of this proposal again to enter on the investigation. He now took into account the possible eccentricity of the disturbing planet to the first order, retaining the same assumption for mean distance. For the modern observations, the tabular errors used, as far as 1830, were exclusively those of the Greenwich Observations, except one by Bessel in 1823; after 1830, the Cambridge and Greenwich determinations and those in the *Astronomische Nachrichten*, were used. Those for the observations anterior to the discovery of the planet were taken from Bouvard.

* This paper was presented to the Society on the evening of Nov. 13, 1846.